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The Cryosphere Discussions

Interactive comment on "Dynamics of Large Pelagic Ice Crystals in an Antarctic Ice Shelf Water Plume Flowing Beneath Land-Fast Sea Ice" *by* Craig Stevens et al.

Craig Stevens et al.

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Response to Anonymous Referee 1 Received and published: 11 November 2020

Au: We thank the Reviewer for the time and effort in commenting on our manuscript and pleased they found it an interesting topic.

We respectfully believe the Reviewer misunderstood the novelty of the dataset – the crystals that we focus on are free-floating and relative to "normal", very large. While the manuscript was clear on these points, we have made some additions to further emphasize this point.

The Reviewer doesn't like the clear separation of Results and Discussion we have chosen here. This is a style choice and we don't agree with the Reviewer's statement that it turns the manuscript into a field report. We are seeking to synthesize independent measurements to draw a conclusion based on multiple information channels. It is notable that the Reviewer makes no comment on the very difficult to obtain turbulence data that provide one of the key datasets and substantial novelty.

The Reviewer dismisses a number of points but provide no references to support their claims. In addition, in several places the Reviewer misrepresents the manuscript by criticising things we didn't say or saying we didn't do things and we clearly did.

The Review paradoxically suggests only a small component of the work being on-topic but then questions why there is no wider context. We attempt to respond to this below.

A number of points are of course helpful, and we respond to these as best we can below.

Rev1: General comments: This manuscript attempts to address 4 key questions around the growth, movement, and aggregation of large ice crystals or platelets in the water column as stated at the end of the introduction. The first two key questions related to the ice crystal size and their dependence on turbulence could in principal be addressed to some extent by the data and methods presented. However, the analysis lacks depth and conclusions seem largely unsupported. Much of the analysis and description are qualitative rather than quantitative and choices of depth levels, profiles, example images, ranges, etc. seem somewhat arbitrary.

Au: We are not sure why the Reviewer would make these statements – these are the first quantitative description of crystal sizes in the region we are aware of (there is nothing out there like Fig 4b we are aware of). We provide an extensive data record of turbulence beneath the sea ice (something rarely seen) and we provide a quantitative in situ estimate of crystal rise speed. We have added in some clarifying phrases around sampling choices.

Rev1: The results are structured in the order of the instruments that were deployed rather than addressing the scientific question. In many instances I am missing the context of a specific measurement and its overall use to support and address the key questions.

Au: Our present approach is common-place and, in this case, is required. We have added a clarifying sentence prior to the Questions in the Introduction.

Rev1: The first key question is in my view not really novel, as large ice platelets have been observed in this region before.

Au: The Reviewer is going to make such a sweeping comment they need to provide some references. They do not. Can they provide a reference that quantitatively identifies large free-floating crystals and measures aspects of their behaviour over any reasonable duration? Can they point to equivalent figures to Figs 4c, 5, 6, 7, 8 or 11?

It is clear that the Reviewer did not appreciate that the crystals are free floating. We used the term "pelagic" in the title and they question this later on. However, we use plenty of other ways of identifying this critical point of novelty including (i) 3rd line of abstract, (ii) a photograph, (iii) the second sentence of the Discussion, (iv) the very un-ambiguous Fig. 12... etc. It is worth noting that the recently published Frazer et al. GRL 2020) did not mention such crystals because they followed the false paradigm that these crystals don't exist in the water column.

Reviewer 2 seemed happy with the novelty and correctly suggests we need to reference Cheng et al 2019 (TC 2019). This recent modelling study notes a number of times they are limited by a paucity of data.

Rev1: The second key question of how the crystals and their deposition depend on the vertical motion and turbulence would be novel, but the analysis appears not to be conclusive and provide new insights.

Au: It is not clear on what basis the Reviewer makes these statements as they pro-

vide no references for where this work has been done previously – we measured the turbulence in the boundary layer and we measured the rise rate of the unusually large crystals and we measured the crystal sizes. Each of these processes has some variability which we quantified so we then put the combined measurements in a mechanistic framework and developed a non-dimensional approach to consider their behaviour. We provided conclusive comments about the range within which the behaviour falls.

Rev1: The third and fourth key question on the source of crystal growth and its influence on the large scale are not even addressed by the analysis and only part of a somewhat inconclusive literature discussion.

Au: We see that Reviewer Two didn't appreciate the sediment question and Discussion so we will remove this question along with some text and merge the rest. We do note that this concept is one the community is struggling with (Hoppmann et al 2020) and so circumstantial evidence would help guide the community to design the next sampling. We don't know what a "somewhat inconclusive literature discussion" means? We find it confusing to at once be criticised for not providing enough context but then also for examining how these data fit into a wider system context.

Rev1: Overall, while being an interesting research topic, the analysis presented in this manuscript lacks scientific insight, support for its arguments, and a clear formulation of its conclusions. The manuscript suffers from poor scientific writing with numerous incomplete thoughts and speculations. Rather than guiding the reader through the story line, results, and argumentation, the text lacks clarity and context. In many instances the context is implied rather than explicitly formulated, which makes it overall very difficult to read and understand the manuscript. It reads more like a report from the field campaign rather than a scientific paper.

Au: These are difficult criticisms to respond to given the Reviewer didn't understand the title or second sentence of the abstract or the first sentence of the Discussion or the very clear schematic diagram at the end or provide any references to back up their

claims.

We are not sure what the Reviewer means by a “story line”? We have some unique data and attempt to put them into a mechanistic and geographic context. It is unclear what the Reviewer means by “lacks clarity”. We identify questions, we provide a description of the data collected in order to do our best to answer the questions. Then we do our best to connect the data to the questions and provide answers where we can.

Rev1: One possible recommendation to the authors would be to make better use of the echo sounder data to only address their question 2, which seems to be the only question that can be addressed with the presented data. They could provide a more in-depth assessment of uncertainties and more information of the apparent classification that has been performed, the particle size estimation from these data, and the attribution to ice crystals vs. other particles in the water column. There is a lot of unnecessary discussion that could be substantially reduced to strengthen the focus on this aspect of the paper and provide supporting evidence.

Au: We have added extra details on the echo sounder data relating to the details of the velocity estimation. We were surprised that the Reviewer made no comment on the turbulence data as being of significant value as the trajectories of the crystals are a balance of buoyancy and turbulence. There is little point exploring only one half of the balance to a greater depth.

Rev1: Specific comments: 1. The abstract completely misses any context, problem statement, or conclusions. It is rather a list of the measurements made with some results.

Au: The measurements are sufficiently novel we felt it was sensible to keep this as the focus and not provide some sweeping context. We have added opening sentences for clarity and incorporated responses to the below where constructive.

Rev1: a. Line 11: What is “outflow” referring to? I assume Ice Shelf Water. **Au:**

Amended

Rev1: b. Line 13-14: “Advecting” horizontally or vertically? How can they be advected if they are already in a depositional layer? I assume this is not meant but the writing is ambiguous.

Au: The sentence says “free-floating”. We have modified the sentence in order to make this clear. It now says . . .” From a fast ice field camp, we captured the kinematics of free-floating relatively large (many 10s of mm in scale) ice crystals that were advecting and then settling upwards in a depositional layer on the sea ice underside (SIPL, sub-ice platelet layer).“

Rev1: c. Line 17-18: There is no evidence provided in the manuscript that the flow is really of “tidal” nature. Is 0.1 m s-1 referring to vertical or horizontal velocity? The turbulence in the boundary layer can also result from other factors than tidal flow.

Au: The abstract doesn’t say the flows are of a tidal nature, it says what the tidal speed was. Furthermore, the Reviewer is incorrect that there is no evidence provided. Figure 3 shows the tides and related velocity structure. As well as non-tidal velocities, the manuscript described the impact of non-tidal turbulence including in Fig 12.

Rev1: d. Line 19-21: How do class 1 and 2 differ? Both appear to be large particles. There is no description in the manuscript of how these classes are being derived and how one of them is being attributed to ice crystals.

Au: We wouldn’t describe motile biology as a particle. We added a clarifying sentence in the Discussion section 4.1 as it is beyond the scale of an abstract or the present focus.

Rev1: e. Line 21-23: Large ice crystals (platelets) have been observed before in this region. This is not a new finding. There is no evidence in the paper that these crystals are depositing as compared to growing locally. It is unclear what is meant by “fully grown”. Is there a limit in the size to which the particles can grow? And how is this

determined? There is no evidence in the paper for the evolution of growing particles and an upper limit.

Au: The point is not that there are large crystals but that they are in suspension as clearly indicated. The Reviewer should include a reference if they are going to dismiss a significant amount of work. The evidence that they are depositing is because we observe them in the water and rising upwards towards a boundary where many more are to be found. The details the reviewer requests are beyond what would be expected in an abstract. We removed the phrase about "fully grown".

Rev1: f. Line 23: The histogram (Figure 4b) suggests a larger size than the 30-80 mm reported here in the abstract. The main text states an average of about 100 mm. All these numbers appear to be inconsistent.

Au: Fair point – we now consistently refer to the size range.

Rev1: g. Line 24: The "settlement" is not being clearly addressed in this paper. What are the implications? If there are any they should be explicitly stated here.

Au: We explicitly calculate the rise speed that drives settlement. The implications of having sea ice constructed from platelets are important and the focus of much study but not the focus here. We have modified the closing sentence of the Abstract. The Review is inconsistent as it asks for implications but criticises the inclusion of Question 4 around large-scale implications.

Rev1: 2. The introduction lacks clarity and focus. It is unclear what the gap of knowledge is, how it relates to the larger picture, and how it is being addressed.

Au: The concept of a "knowledge gap" is appealing but it's not really how geophysical science works. We don't know enough to say "we must go and measure process X". We can model the planet now – it's just that is doesn't match what we measure. It is instead a refinement of scales and mechanics sufficient to get the interplaying processes right.

The Introduction opens with the global setting and the first paragraph ends with "This

supercool water drives sea ice growth by absorbing heat into the stratified upper ocean and facilitates the generation and growth of ice crystals". The next paragraph opens with unknowns about the crystals. This ultimately leads to four questions one of which is what the knowledge gap is and another is essentially "how it relates to the larger picture" which we don't presage before we actually do the science. They have a balance moving from discovery through to implications. This is quite focused. We have re-worked aspects of the Introduction to see if we can meet the Reviewer's expectations.

Rev1: a. Line 28-29: I am not sure how Antarctic sea ice variations are "confounding communication of key issues to stakeholders and decision makers" and what the formation of platelet ice has to do with it? This argument seems a bit arbitrary and poorly motivated.

Au: Stakeholders want certainty and a sense we can model the earth system accurately. We, as a community, can't do this at present. However, expanding on this won't help the focus. We have added a linkage phrase and a reference to help clarify this important point for the Reviewer.

Rev1: b. Line 29-30: The warming on the deep water that is responsible for the increased melting of some of the ice shelves has not yet been attributed to "anthropogenic" sources. Thus, this statement is not quite correct. In addition, I do not know of any evidence that the Ross Ice shelf and the study site have been affected by this process.

Au: The text said neither of these things.

Rev1: c. Line 35: Not all ice shelf water rises buoyantly to the surface. In some regions the water flowing out of the cavity is actually denser than the surrounding water and sinks along the bottom (e.g. in Filchner Trough).

Au: A bottom-following ice shelf water plume must be unusual. A reference would

help.

Rev1: d. Line 47-48: The relation between ice growth, viscosity, and advection is unclear in this sentence.

Au: We have added a clarifying phrase. “If these crystals grow slowly, remaining sufficiently small that viscosity dominates buoyancy so that rise rates are very slow, then they are mainly passively advected.”

Rev1: e. Line 53-56: It is unclear what is meant with this sentence.

Au: We have clarified this sentence. “Despite the challenges in making measurements in this environment, one correlation that emerges is that SIPL thickness and supercooled seawater are co-located (Langhorne et al., 2015; Brett et al., 2020).”

Rev1: f. Line 65-74: The purpose of this paragraph is unclear. I assume that it aims at providing context for ice-nucleation particles, but this is not clear to the reader at first. In addition, there seems to be irrelevant information and colloquial language. The reasoning implied by the last sentence is not clear to the reader. Isn’t it well known by in-situ observations that there is supercooled water under the ice shelf? What is the relation between the supercooled water and the marine sediments at the surface of the ice shelf?

Au: The paragraph is to provide part of the “story line” asked for elsewhere. It provides the context for the geographical location and the datasets that allowed us to arrive at the point that we could collect the observations that we did.

Which language is colloquial? The text in quotes come from the literature as referenced. We have added some text to aid in understanding around the relationship between the supercooled water and the marine sediments at the surface of the ice shelf.

Rev1: g. Question 1 is already known. There are large ice platelets in this region. Question 2 seems like the most appropriate for this study. Questions 3 and 4 are not

addressed by this study.

Au: As above, the Reviewer does not provide a useful reference for evidence of large free-floating crystals and we do not believe they have been adequately described previously. The Review includes multiple requests for context such as that provided by the Discussion of Q4.

Rev1: 3. The method of how the vertical velocity is being derived from the echo sounder data is not sufficiently described to be reproducible (lines 134-141). How large are the vertical segments? How are features identified and tracked? How well does the feature tracking from one time step to the next perform and what is the estimated error? The meaning of the sentences in lines 138-141 is unclear and needs clarification.

Au: These are useful clarifications. We have added some extra text and a reference here on the analysis. One of the neat aspects is the improvement in vertical resolution through vertical low-pass filtering. It is notable that the Reviewer focuses on this half of the mechanics and misses the important contribution from the turbulence data and the intriguing results around how the turbulence distribution doesn’t perfectly match a friction driven boundary-layer.

Rev1: 4. The importance and relevance of the background conditions described in section 3.1 for addressing the key questions is not evident from the text. How do the T and S profile, and current measurements affect where and how the ice crystals could grow in the water column? What is the role of different flow speeds at depth and close to the surface? Why are the depth levels chosen this way? How does the rising of the ISW plume cause a shear in the flow (line 175-176)? Doesn’t the below surface freezing point temperature throughout the water column suggest that ISW is present in the entire water column? How else would it be possible to form waters with a temperature below -1.9degC down to 500m?

Au: These are interesting questions but not all relate to the focus at hand. The thick ISW layer is well known for this region (Robinson et al 2014) but with crystals forming

beneath the surface we are interested in insitu supercooling. We provide some extra information and clarify the point about the ISW plume in terms of what we actually said.

Rev1: It is unclear why tides should be the main driver of the horizontal flow (lines 176-181)? How about pressure/density gradients, wind forcing, and large-scale circulation?

Au: It is not clear the Reviewer read the text. It said "The deepest current meter provided the best quality current speed results (Figure 3a,b). The upper current meter did work for a few days at the beginning and was sufficient to show that the upper 175 speeds were between 50 and 100

Rev1: 5. Section 3.2 contains many unsupported statements. a. There is no apparent evidence for the reader and no scientific analysis provided in the manuscript that there is a "constant supply of crystals from depth" or their size (lines 188-189).

Au: This is why we provide the Results-Discussion structure as we do. We have 10 days of day sampled constantly with modest signal fluctuations. The text now references Fig 3e and we removed the phrase about scale.

Rev1: b. The authors do not provide evidence for the claims made in lines 190-195.

Au: We provide a Figure and a statement about what we are seeing in the figure and return to this in the Discussion. We could add arrows in the figure or mention a sequence of specific times? The Reviewer needs to provide more detail about what they would like to see in terms of evidence in order for us to extend this point.

Rev1: c. How is it known what fraction of the signal derived from the echo sounder results from ice crystals (line 196-197)?

Au: We don't claim to be able to quantify this. It remains a good question though. It doesn't impact our analysis because it responds to the peaks in backscatter anyway.

Rev1: How is it known that the signal also includes biological particles?

Au: We suggest they are biological in nature because they look like they're swimming

erratically and across the wider motion. The text now explicitly states this.

Rev1: How is the background defined (lines198-201)?

Au: This is essentially the same point as above where we noted the present analysis does not need to explicitly identify this separation to calculate the vertical motion. Instead the analysis is primarily responsive to the larger signals in the segments.

Rev1: There is no evidence provided for rising particles (line 201-203).

Au: Figure 7 shows an average rise speed. We can include video of crystals rising in the supplementary information if that would help.

Rev1: No analysis of video sequences supporting the identification of ice crystals (line 203) is provided.

Au: Correct -this is why we analysed the acoustic information which has a much better quantitative basis.

Rev1: d. Is the positive shift of the histogram towards an upward directed velocity statistically significant and larger than the uncertainty (lines 204-205)? If not, no such claim can be made.

Au: We have clarified the text on the reliability of the measurements which is aided by (i) pre low-pass filtering and (ii) averaging over the 10 days of sampling.

Rev1: e. How is the crystal size (7 cm, line 205) of the particles in the echo sounder estimated? Why is no histogram provided for those particles? What is the range of Reynolds numbers given the range in velocity and particle size?

Au: We re-wrote the Reynolds number paragraph to clarify these points. It now says "Considering the Reynolds number (Re=characteristic velocity x dimension/kinematic viscosity) as quantifying the balance of inertia and viscosity, and with the larger crystals being on average around 10 cm in diameter and rise speeds of the order of 1 cm s-1, this implies a Reynolds number ranging from 50-2000 with an average of 1000."

Rev1: f. What is the depth range over which the echo sounder data is evaluated (line 196ff)?

Au: The depth range shown in the Figure is representative of the usable data. The methods now explicitly states this depth in Section 2.3.

Rev1: g. Lines 213-226: It is unclear how the discussion of the temporal evolution in these signals relates to the question addressed in the paper.

Au: The temporal evolution relates to the consistency of supply which relates to the questions about presence, dynamics and wider implications.

Rev1: h. It is unclear how the conclusion is drawn that the "current meter spectrum is dominated by the tide" (line 228-229) and how this relates to the issue addressed in the paper. What is the role of the frequency spectrum for the ice crystal formation and transport and how is this analysis motivated?

Au: The Reviewer makes a fair point about the tides and spectrum. While the statement in the text was true, the displayed spectrum was derived in a way that provided best reliability in the upper frequencies as stated in the text and consequently did not resolve the tidal peak. This statement has been modified. The spectrum is there to see if there are any dominant modes of variation or if it is a continuum of scales in the driving velocity signal – and it is largely the latter. The text now says . . ."The current meter spectrum is constrained to lower frequencies with much of the spectrum above 50 cpd reaching an apparent noise-floor implying that the variations seen in temperature and backscatter are not advection-driven."

Rev1: 6. Line 265: There is no evidence provided in the paper for the visual observations referred to here.

Au: Figure 5 provides this evidence. We now explicitly reference Fig 5.

Rev1: 7. Line 277-279: This sentence is not supported by the analysis. There is no evidence provided for the upward transport of large ice crystals and their origin.

Au: The unique analysis in Figure 7 is that evidence.

Rev1: 8. Line 280-282: How is this separation obtained? Why is there no analysis showing the statistics of the different classes? What criteria are used to separate these classes.

Au: We have clarified the language here and in the Results. The separation of Class 1 is behavioural and there is probably a continuum between classes 2 and 3 but our acoustic sampling is dominated by the larger end of this combined class. However, the statistics of the different classes is not the focus here. Class 1 are biological in nature and Class 3 is well covered by Frazer et al. 2020. It is Class 2 that is not thought to exist in suspension that is the contribution here. The focus of the manuscript as identified in the Questions is around the large crystal behaviour in the turbulence of the ice shelf plume.

Rev1: 9. Line 396: There is no evidence provided in the manuscript for a substantial sediment load in the region of ice crystal formation.

Au: We have moved or deleted the sediment text in keeping with both Reviewer's comments. We do note however, that the field site is down-wind of the geographic feature called the "dirty ice" and we did provide circumstantial evidence – ". A hot-water cutter was used to melt through and remove the sea ice in blocks. It was notable that upon removal of the blocks, the water which filled the hole appeared milky but that this gradually dissipated over the subsequent days. After 12 days of operations and many seal occupations of the holes, the hole water was fully flushed and very clear. We speculate that the water in the hole was initially from the melting of the sea ice and upwards drainage from the SIPL and contained sufficient levels of sediment to be visible but that over time this was replaced with clear ocean water".

Rev1: 10. The manuscript is missing a clear formulation of the conclusions at the end.

Au: We thought having the closing thoughts as part of the large-scale context question was sufficient. However, motivated by the comments from the Reviewer we have added a “concluding remarks” subheading.

Rev1: Technical comments: - “Pelagic”: I am confused by the use of this word in the context of ice crystals. It is typically used for marine habitats and (if I am not mistaken) is a greek word for “open ocean”. So, what is the intention of classifying ice crystals as “pelagic”? I thought the key aspect of the paper was to look at large size of the ice crystals occurring in this region. –

Au: We used “pelagic” in the sense of away from boundaries. In order to avoid confusion we can retitle using the term “suspended” but even this has its issues as it implies that there is no net rise which is counter to our analysis. We could put “pelagic” in quotes?

Rev1: Line 48-49: “thought to exist” requires a reference. –

Au: This was all text associated with the recent review by Hoppmann and the Frazer paper both referenced at the start of the paragraph. We now repeat the references.

Rev1: Line 78-80: These claims require references. –

Au: This was all text associated with the recent review by Hoppmann referenced at the start of the paragraph and nicely synthesizing a number of studies and providing a nice context for this present work. We now repeat the references.

Rev1: Line 187: there is no figure 4c –

Au: Corrected thanks

Rev1: Line 409-410: A quote requires accurate referencing.

Au: We were returning to a point made earlier which was accurately referenced. We have repeated the reference.

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